The meteor of April 8 was directed from a radiant in the eastern limits of Virgo, and not far from Spica. A fireball was seen on March 16 last, which was probably from the same radiant, as the paths converge on the point 205° – 18°. This region is the centre from which many fireballs and ordinary shooting stars are directed in April and other months, as the following table will prove :—

Radiant point	Date.	Description	Observer or authority
210 – 6	January 5-11, 1870	Meteor shower	Tupman
205 – 8	January 20-22, 1877	,, ,,	Denning
200 – 5	January 20—Feb. 3, 1896	,, ,,	Herschel
204 – 10	January 21—Feb. 23, 1869	,, ,,	Denning from Tupman's obs.
202 – 9	February 13, 1869	,, ,,	Tupman
210 – 13	February 15-21, 1877	,, ,,	Denning
200 – 10	March 2-3, 1870	,, ,,	Tupman
205 – 18	March 16, 1896	Fireball	Denning
204 – 10	March 23, 1895	Meteor shower	Denning
204 – 8	March 31—April 12, 1872	,, ,,	Denning from Italian obs.
210 – 10	April 7-16, 1877	,, ,,	Denning
216 – 10	April 11, 1871	Fireball	Niessl
206 – 8	April 12-26, 1879	Meteor shower	Sawyer
210 – 7	April 1877	,, ,,	Corder
209 – 3	April 1874	,, ,,	Denning
209 – 9	April 1896	,, ,,	Herschel
198 – 8	April 18, 1841	,, ,,	Forshey
214 – 13	April 21, 1887	Fireball	Niessl
218 – 5	April 21, 1889	Bright meteor	Denning
210 – 9	April 19-23	Radiant 10 fireballs	,,
207 – 7	April 22, 1876	Meteor shower	,,
206 – 9	April 27, 1851	Fireball	Niessl
209 – 8	May 3-15, 1872	Meteor shower	Denning from Italian obs.
214 – 7	May 12, 1878	Fireball	Herschel
214 – 7	May 29, 1889	,,	Denning
217 – 6	July 7, 1895	,,	,,

The mean of the twenty-six positions is 209° – 9°.

The fireball of April 12, 1896, came from a radiant in the N.W. sky at 50° + 42°. The large meteor of April 22, 1894, had a similar radiant (*Observatory*, June, 1894), and the same may be said of the fireball of March 9, 1875.

Bristol, April 27. W. F. DENNING.

Becquerel and Lippmann's Colour Photographs.

I WISH to raise a point in connection with the optics of photochromy, which was not touched upon at the recent discussion at the Royal Society. The photochromatic spectra produced by the earlier workers, and especially by E. Becquerel about 1850, have long been known and have always appeared to be very mysterious to those who have repeated the experiments. When Prof. Lippmann's success with the interferential method was made known some five or six years ago, and his first results exhibited in this country, many of those who were acquainted with the previous methods of producing coloured spectra by direct impression came to the conclusion that all the earlier workers had unconsciously been producing the Lippmann effect. This supposition was not unreasonable. In Becquerel's method, for instance, which gave the most brilliant effects, the sensitive film of violet chloride is produced on a surface of metallic silver, and is thus backed by the necessary reflecting surface. Even when the colour sensitive chloride is on paper, as in the still earlier experiments of Robert Hunt and Sir John Herschel, it is not unreasonable to suppose that the bounding surface of the paper and silver haloid reflects sufficiently well to produce the necessary interference. At the discussion following Prof. Lippmann's paper, Lord Rayleigh raised the question whether the earlier and later results were not due to the same cause, but there seemed to be an impression that the Becquerel and Lippmann effects were produced by different causes. For my own part, I am bound to confess that the reasons assigned for arriving at this decision still appear to be inconclusive. The main points which have been allowed to prevail are that the Becquerel photographs cannot be fixed, that they appear of the same colour at whatever angle they are viewed, and that they appear of the same colour by transmitted and by reflected light. The fact that these photographs cannot be fixed is easily explained if we bear in mind that the silver salt, is not embedded in a vehicle, as in Lippmann's process, and that there is consequently nothing to hold the laminæ apart at the correct intervals when the fixing solution has done its work. The other points are less easy to explain ; but it may be suggested that the difference is here due to the earlier experimenters having used coarse-grained films, in which the silver haloid particles are sufficiently large to *scatter* the colours produced in the film by the laminated structure of the alternating planes of decomposition and no decomposition. The question is a purely physical one, and may be put into the following form :—If the Lippmann effect is produced in a coarse-grained instead of in a transparent film, would not the Becquerel results be obtained ? If physicists can answer this in the affirmative, the difficulty of supposing that similar results can be obtained by totally different causes would disappear.

R. MELDOLA.

Aquatic Hymenoptera.

UNDER the title "On Two Aquatic Hymenoptera, one of which uses its Wings in Swimming," Sir John Lubbock, Bart., read a paper before the Linnean Society, May 7, 1863, therein describing two most extraordinary insects, which he named *Polynema natans* and *Prestwichia aquatica.*

Last year I had the good fortune to obtain a large number of both sexes of the first named, which, after most critical microscopic examination, I identified as belonging to Haliday's *Caraphractus cinctus*, the unique characteristic of the "*keeled* metathorax" placing the matter beyond a doubt. The late Prof. Riley, to whom I had the pleasure of showing specimens, fully confirmed my opinion, as also did Mr. Charles Waterhouse.

The life-history of any of these minute Hymenoptera is not worked out in one season—very far from it ; and since last year I have steadily followed up the chain of facts, my efforts being again rewarded by finding this most exquisite Hymenopteron this season within twenty miles of London.

Encouraged by my success, I continued my search for some hours at a small pond, and at last captured two female specimens of the long-lost-sight-of *Prestwichia aquatica* (Lubbock), which has not been recorded since its first capture by Sir John Lubbock in 1862—thirty-four years ago !

The two specimens (and *I*) have scarcely taken any rest since their capture yesterday morning, May 4 ; but they have been constantly running or paddling *under* water, never once having been to the surface. When I first put them into the tank, they had the *greatest* difficulty in forcing their way through the film ; but as soon as that was accomplished, they moved about with their legs, as propellers, far more rapidly than did *Caraphractus cinctus* with its wings.

I am looking forward to capturing the *male Prestwichia aquatica*, which has not yet been recorded by any entomologist.

21 Manor Gardens, Holloway, N. FRED. ENOCK.

Dalton's Atomic Theory.

IN the review of "A New View of the Origin of Dalton's Atomic Theory," published in your issue of April 16, your reviewer, in summing up the evidence as to the origin of the atomic theory, makes an omission of such importance that it cannot be allowed to pass unchallenged. He attaches great weight to Thomson's statement that in 1804 Dalton himself informed him "that the atomic theory first occurred to him during his investigations of olefiant gas and carburetted hydrogen gas." Now these researches, as pointed out by your reviewer, were begun in the summer of 1804, a date which is assigned to them by Dalton himself, and is confirmed by the entries in his laboratory note-books of the time ; so that Thomson's statement amounts to saying that the atomic theory first occurred to Dalton in the *summer of 1804.* This conclusion appears to us to be entirely discredited by the fact that several detailed tables of atomic weights and lists of atomic symbols, which are dated *September 1803,* occur in Dalton's laboratory note-books, one of these tables being reproduced in facsimile at p. 28 of the work under review, but not referred to by your reviewer.

It must be remembered that Thomson's account of the origin of Dalton's theory was first published in his "History of Chemistry" (vol. ii. p. 291) in 1831, no less than twenty-seven years after his visit to Dalton had been paid. Moreover, in 1850, after the lapse of another nineteen years, he gave a second and totally different account of the origin of the same theory, saying it was founded on the analysis of protoxide and dentoxide of nitrogen (Henry, "Life of Dalton," p. 80).

THE AUTHORS.

THE question is whether Dalton was led to apply the Newtonian doctrine of atoms to the explanation of chemical

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phenomena (1) by a consideration of the composition and properties of atmospheric air; or (2) in consequence of remarking the results of the analysis of certain pairs or series of chemical compounds, the composition of which illustrates the law of multiples.

The authors contend for the former view, and adduce the contents of the lecture note-book dated 1810; but these notes of lecture 17 contain evidence of confusion in the statements made by Dalton himself. In these notes he says (p. 14 of the book), "In order to reconcile or rather adapt this chemical theory of the atmosphere to the Newtonian doctrine of repulsive atoms or particulars, I set to work to combine my atoms upon paper," &c. (P. 15), "In 1801, I hit upon an hypothesis." This hypothesis relates to the mutual repulsion of gaseous particles. (P. 16), "Upon reconsidering this subject it occurred to me that I had never contemplated the effect of difference of size. . . . This idea occurred to me in 1805." (P. 17), The different sizes of the particles being once established, "a train of investigation was laid for determining the *number* and *weight* of all chemical and elementary principles which enter into any sort of combination one with another."

So that the atomic theory as applied to chemical combination took shape in Dalton's mind according to this version of the story in 1805. Yet according to another of the note-books, quoted p. 26, he was using symbols to express the atoms of elementary bodies in 1803. The authors notice this conflict of statement, but get rid of it by assuming 1805 to be a clerical error for 1803.

Thomson was probably wrong in attributing the origin of the atomic theory to the study of marsh gas and olefiant gas. But in his exposition of the Daltonian doctrine, prepared only a short time after his interview with Dalton, he illustrates the use of the atomic doctrine by reference to the oxides of nitrogen. This was in his third edition, published 1807. In his sixth edition he introduces the oxides of carbon as well as the oxides of nitrogen. Thomson, therefore, from the time of his interview with Dalton retained the impression that the genesis of the theory was intimately connected with the facts known to Dalton as to chemical combination in multiple proportions, though he was evidently not clear as to the particular case first considered. That it was the oxides of nitrogen which first attracted Dalton's attention is, however, probable from the fact that he refers to them in the following noteworthy passage which occurs in his paper on the atmosphere read at Manchester, November 12, 1802: "These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity." The authors have succeeded in discrediting the story about marsh gas, but it still remains doubtful whether Dalton's recollections in 1810 of what occurred six or seven years before are more trustworthy than the impressions of Thomson received much earlier, when it is a question as to the order in which various considerations came before his mind in the long course of meditation which led to the adoption of his theory.

YOUR REVIEWER.

An Advance in Röntgen Photography.

SINCE my last communication I have been pursuing the study of the photography of the soft tissues in the living adult subject, and making attempts to see shadows of them on the fluorescent screen. In a previous communication I was able to state that I had accomplished these in the region of the neck, the tongue, hyoid bone, larynx, &c. Proceeding downwards, I have now photographed and seen shadows of the cardiac area. In the photograph the diaphragm is clearly indicated below; the pyriform shape of the cardiac area is well made out, the base downwards, apex upwards, and the right and left borders show the relationship to the spine and ribs.

JOHN MACINTYRE.

179 Bath Street, Glasgow, May 9.

PROJECTS FOR ANTARCTIC EXPLORATION.

ON January 28, 1841, Captain James Clark Ross and his comrades on her Majesty's ships *Erebus* and *Terror*, saw for the first time the giant volcanoes, rising in latitude 78° 30′ S., which bear the names of the only vessels that ever sighted them.

Fifty-five years later we remain in possession of no more information regarding these regions than was brought home by the discoverer. This is a circumstance absolutely unique in the modern history of geography. During these fifty-five years the map of Africa has developed from a *carte blanche* into a well-ordered delineation of mountains, lakes and rivers, even towns and villages the names of which are household words. In the far North the limits of the unknown have been and are still being strenuously pushed back. It is only in the far South that the explorer's march has been stayed, and during the last ten or fifteen years the importance of securing a farther advance in this direction has been given expression to, with increasing frequency and emphasis, by the scientific men of all countries. The latest and most weighty statement on the subject was the resolution of the Sixth International Geographical Congress, drafted in London by the leading geographers of Europe, to this effect:

"That the Congress record its opinion that the exploration of the Antarctic regions is the greatest piece of geographical exploration still to be undertaken. That, in view of the additions to knowledge in almost every branch of science which would result from such a scientific exploration, the Congress recommends that the scientific societies throughout the world should urge, in whatever way seems to them most effective, that this work should be undertaken before the close of the century."

It is clear to all scientific men that, although the recent experimental trips of Scottish and Norwegian whalers to the Antarctic regions have led to some distinct advances in our knowledge, and have rightly occupied a good deal of attention, they leave the question of serious exploration untouched. In the absence of a real expedition, we must continue to eagerly utilise every scrap of information which may be obtained by any means; but such trifles are only of provisional value. The drowning man may, for want of other floats, catch at straws, but the least critical spectator of this proverbial tendency would not argue that a life-belt was therefore unnecessary.

Antarctic exploration, if newspaper reports are to be trusted, has been commenced by the American Dr. Cook, who accompanied Lieutenant Peary on one of his journeys in Greenland, and has now got together a small scientific party on board two little sailing vessels of only 100 tons, with which he hopes to penetrate to the coast of Graham's Land and winter there. Weddell, in 1823, succeeded in reaching 74° 15′ S., in that neighbourhood, one of his vessels being only 65 tons, so that small size does not necessarily mean failure. If Dr. Cook has experienced ice-navigators with him, he will probably be able to effect a landing and collect some useful information. His equipment, however, is, we fear, inadequate to the task he has undertaken, and much must not be expected from it.

A more serious effort is announced as almost ready. It is to be commenced in September this year, under a flag which we believe has not hitherto appeared in polar regions, that of Belgium. The proposed expedition is being arranged by Lieut. A. de Gerlache, of the Berlin Navy, under the auspices of the Royal Belgian Geographical Society, and the expenses, which are estimated at £10,000, are to be met by public subscription. It is said that a large proportion of the money has been promised, the Brussels municipality have voted a grant; but until the whole of the cost is guaranteed, it would be rash to look upon this or any other expedition as a settled affair. A strong scientific staff is intended to accompany the vessel, which will probably try to get south along the east coast of Graham's Land.

In Germany the enthusiasm for Antarctic exploration has been gradually rising, and a strong Committee was appointed at the Eleventh German Geographical Congress, held last year at Bremen, to organise an expedition.

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